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Learning to Love Globalization:

Education and Individual Attitudes Toward International Trade

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Abstract

Recent studies of public attitudes toward trade have converged upon one central finding: support for trade restrictions is highest among respondents with the lowest levels of education. This has been interpreted as strong support for the Stolper-Samuelson theorem, the classic economic treatment of the income effects of trade which predicts that trade openness benefits those owning factors of production with which their economy is relatively well endowed (those with skills in the advanced economies) while hurting others (low skilled workers). We re-examine the available survey data, showing that the impact of education on attitudes toward trade is almost identical among respondents in the active labor force and those who are not (even those who are retired). We also find that, while individuals with college-level educations are far more likely to favor trade openness than others, other types of education have no significant effects on attitudes, and some actually *reduce* the support for trade, even though they clearly contribute to skill acquisition. Combined, these results strongly suggest that the effects of education on individual trade preferences are not primarily a product of distributional concerns linked to job skills. We suggest that exposure to economic ideas and information among college-educated individuals plays a key role in shaping attitudes toward trade and globalization. This is *not* to say that distributional issues are not important in shaping attitudes toward trade – just that they are not clearly manifest in the simple, broad association between education levels and support for free trade.

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I. Introduction

A growing body of scholarly research has examined survey data on attitudes toward trade among voters, focusing on individual determinants of protectionist sentiments. These studies have converged upon one central finding: fears about the distributional effects of trade openness among less-educated, blue-collar workers lie at the heart of much of the backlash against globalization in the United States and other advanced economies. Support for new trade restrictions is highest among respondents with the lowest levels of education (e.g., Scheve and Slaughter 2001a, 2001b; Mayda and Rodrik 2005; O'Rourke and Sinnott 2002). These findings are interpreted as strong support for the Stolper-Samuelson theorem, the classic economic treatment of the income effects of trade which predicts that trade openness benefits those owning factors of production with which their economy is relatively well endowed (those with high skill levels in the advanced economies) while hurting others (low skilled and unskilled workers).

However, the positive relationship between education and support for trade liberalization might also – and perhaps primarily – reflect the facts that more educated respondents tend to be more exposed to economic ideas about the overall efficiency gains for the national economy associated with greater trade openness, and tend to be less prone to nationalist and anti-foreigner sentiments often linked with protectionism (see Bauer, Poole, and Dexter 1972, 103; Holsti 1996, 87---88). Studies have shown that measures of economic knowledge are strongly associated with education levels among individuals, and exposure to economics at the college level has especially powerful effects (e.g., Saunders 1980; Gleason and van Scyoc 1995). And a large body of research shows that increased education – and especially college education – tends to socialize students to have more tolerant, cosmopolitan views of the world (e.g., Campbell et al. 1960, 475--481; Erikson, Luttbeg, and Tedin 1991, 155---6). It thus seems quite possible that differences of opinion among more and less-educated voters over the trade issue may be shaped less by Stolper-Samuelson style calculations about the expected distributional effects of trade than by competing ideational and cultural perspectives – that is, education may be important here primarily because

it teaches students to think about trade and globalization in different ways and/or to evaluate it according to a different set of values.

Which of these very different interpretations is more correct? The answer to this question is critical to our understanding of the political economy of trade and the nature of the political disagreements over globalization now taking place. Here we re-examine the available survey data on individual attitudes toward trade, conducting a simple test of the effects of education on support for trade that distinguishes clearly between the Stolper-Samuelson interpretation of this relationship and alternative ideational and cultural accounts. We find that the impact of education on attitudes toward trade is almost identical among respondents currently in the active labor force and among those who are not (even those who are retired). That the effects of education on trade policy preferences are not mediated by whether or not individuals are actually being paid for the employment of their skills strongly suggests that it is not primarily a product of distributional concerns. The analysis also reveals clear non-linearities in the relationship between education and trade preferences: while individuals who have been exposed to college or university education are far more likely to favor trade openness than those who have not, other types of educational attainment have no significant effects on attitudes and some even *reduce* the likelihood that individuals support trade even though they clearly contribute to skill acquisition. These findings indicate that the particular ideational and/or cultural effects associated with college education, and not the gradual accumulation of skills, are critical in shaping individual attitudes toward trade.

We conclude that the impact of education on how voters' think about trade and globalization has more to do with exposure to economic ideas and information about the aggregate and varied effects of these economic phenomena, than it does with individual calculations about how trade affects personal income or job security. This is *not* to say that the latter types of calculations are not important in shaping individuals' views of trade – just that they are not being manifest in the simple association between education and support for trade

openness. As we discuss in the concluding section, we think it is likely that concerns about the effects of trade on personal income and job security might actually hinge on the particular impact of trade openness in specific industries. One of the key implications of our findings is that future empirical tests of the determinants of individual trade preferences need to be substantially refined to identify the impact of distributional concerns on attitudes towards trade and globalization and distinguish these from the impact of ideational and cultural factors.

II. Explaining Attitudes Toward International Trade

To date, the analysis of survey data aimed at revealing the determinants of individual attitudes toward trade has focused predominantly on occupational differences among respondents. A principal aim has been to test standard economic models that describe the income effects of trade for different individuals as a function of the types of productive inputs they own. Examining data from recent NES surveys in the United States, Scheve and Slaughter (2001a, 2001b) emphasized the importance of respondents' "human capital" or skills (measured principally by years of education), finding that individuals with lower skills were more likely to support restrictions on imports than those with higher skills. Mayda and Rodrik (2005) and O'Rourke and Sinnott (2002) came to similar conclusions after examining the data from the 1995 ISSP survey: again skill levels, measured either by years of education (Mayda and Rodrik) or occupational categories (O'Rourke and Sinnott), were found to have large effects on attitudes, with lower skilled individuals being the most protectionist in outlook.¹ In terms of economic theory, these findings have been interpreted as providing strong support for the Stolper-Samuelson theorem (1941), which predicts that trade raises real incomes for those who own factors with which the

¹ Similar types of findings are reported by Balistreri (1997) and Beaulieu (2002), who examined data on support for the Canadian-US Free Trade Agreement in Canada, by Gabel (1998), who used *Eurobarometer* data on attitudes toward European integration, and by Baker (2003) using similar survey data on attitudes toward trade in Latin American nations.

economy is relatively well endowed (i.e., labor skills in the developed economies), while disadvantaging owners of other factors (unskilled or low skilled labor).² Scheve and Slaughter highlight as a “key finding” of their study the conclusion that “the preferences about trade and immigration policy align strongly with labor market skills “(2001b, 9). Mayda and Rodrik report that the results from their analysis of the importance of educational attainment in shaping views about trade are “strikingly supportive of the implications of the factor-endowments model and of the Stolper-Samuelson theorem” (2005, 1409).

Yet there are other, very plausible, ways to interpret the positive relationship between the education levels of surveyed individuals and their support for trade openness. It seems particularly limiting to consider education only as a measure of workers’ skill levels, relevant only in the way it affects the expected income effects of trade for each individual. Education may be relevant here for *other* reasons – *other* causal mechanisms may be generating the observed association between education levels and pro-trade attitudes. We think there are two likely candidates: an *ideational* mechanism that hinges on the role played by economic ideas and knowledge in the formation of economic policy preferences; and a *cultural* mechanism that relates commitments to key values, such as tolerance and cosmopolitanism, to attitudes toward foreign policy issues.

The *ideational* argument is fairly straightforward. Highly educated respondents are likely to think about international trade in very different ways compared to less-educated counterparts, using a more sophisticated set of ideas about cause-and-effect relationships and more information

² This theorem has been used extensively in the analysis of trade politics; see Rogowski 1989; Hiscox 2002.

about the effects of trade for themselves and for others.³ College-educated individuals, in particular, are likely to be far more informed than others about the aggregate efficiency gains associated with expanded trade, especially if they have had any contact at all with economics courses and with the theory of comparative advantage. While there is a firm consensus among economists on the virtues of trade openness, the counterintuitive loveliness of the law of comparative advantage makes it much more difficult to convey the case for trade outside the college classroom (see Krugman 1993). Studies of economic “literacy” among the public have shown that general measures of economic knowledge are strongly associated with education levels among individuals, and college education in particular (e.g., Saunders 1980; Gleason and van Scyoc 1995). Exposure to at least one college-level economics course is an especially powerful predictor of economic knowledge. And critically, related studies show that higher levels of economic knowledge among surveyed individuals have large positive effects on support for free trade (see Walstad 1997; Walstad and Rebeck 2002).⁴

This type of ideational argument does have precedents in both the international political economy and broader international relations literatures. Ideas, viewed as beliefs about cause-and-effect relationships, have been assigned key roles in accounts of policymaking in a variety of

³ Among scholars who have examined surveys of public attitudes toward trade, the topic is typically regarded as a complex issue about which most survey respondents have very low levels of information; see Bauer, Pool, and Dexter 1972, 81---84; Destler 1995, 180; Pryor 2002.

⁴ Walstad and Rebeck (2002) make the larger point that scholarly analysis of public opinion on economic issues makes the erroneous implicit assumption that survey respondents are undifferentiated in terms of their economic knowledge. In fact, individuals differ dramatically in their levels of economic knowledge, as measured by their scores on a set of test questions covering basic economic concepts and facts, and such knowledge scores are significant predictors of attitudes about a range of economic policy issues.

areas, including international cooperation on environmental issues and arms control.⁵ On the trade issue specifically, scholars have linked the removal of mercantilist restrictions on trade in Europe in the nineteenth century to the ideas of Smith and Ricardo and the birth of classical trade theory (see Kindleberger 1975; Bhagwati 1988). And the multilateral trade liberalization pursued among western nations after 1945 has been connected to neoclassical economic theories and the spread of Keynesian ideas in particular (see Ruggie 1983; Goldstein 1988).⁶ These types of accounts have traditionally focused on the importance of particular ideas among policymakers, and the transmission of ideas to government officials via transnational networks of experts and activists (e.g., Haas 1992; Keck and Sikkink 1998). Examining the distribution of economic ideas among voters, and how this might be connected to policy preferences, might be an interesting and important extension.

Another plausible explanation for the relationship between education and attitudes toward trade focuses on differences in *cultural* values: highly educated individuals are less prone than others to nationalist and anti-foreigner sentiments that are often linked with protectionism in political debates. There is a large scholarly literature showing that education, at least in the United States, tends to socialize students to have more tolerant, pro-outsider views of the world (e.g., Campbell et al. 1960, 475---481; Erikson, Luttbeg, and Tedin 1991, 155---6; McClosky and Brill 1983). Education can foster tolerance, not just by increasing students' knowledge of foreign cultures and raising levels of critical thinking, but also by generating more diverse and cosmopolitan social networks, especially at the college level (see Case et al. 1989; Allport 1954). Indeed, Betts (1988) has argued that one aspect of class identity that emerges among the college

⁵ For a general discussion, see Goldstein and Keohane 1993; on environmental negotiations, see Haas 1992; on arms control, see Adler 1992 and Price 1998.

⁶ See also Hall (1989) for essays on the particular impact of Keynesian macroeconomic theory among policymakers in the 1930s and 1940s.

educated in western nations is a commitment to cosmopolitanism and an appreciation for diverse cultures. And studies of survey data show that various measures of nationalism and national pride are significant predictors of support for trade protection (see Mayda and Rodrik 2005, 1414---6; O'Rourke and Sinnott 2002, 173).⁷

This type of argument resonates with a growing body of research in international relations that focuses on the importance of cultural values and conceptions of identity and how they shape the interests pursued by policymakers in international affairs – in areas including military strategy, humanitarian intervention, and support for human rights.⁸ It is more difficult to find cultural accounts of trade politics or policymaking, specifically, in the political economy literature, though references to the popular appeal of protectionism when linked with nationalism and xenophobia are common (e.g., Bauer, Poole, and Dexter 1972, 103).

How then should we interpret the observed connection between education and support for trade? Besides the standard account, which focuses on Stolper-Samuelson style distributional concerns, alternative accounts that stress the importance of economic ideas and related values also seem highly plausible. We suggest a very simple test to establish whether the education connection is primarily reflecting concerns about the effects of trade on respondents' personal incomes, or whether it is a manifestation of broader differences in ideas and/or values among surveyed individuals. We examine the impact of education levels on attitudes toward trade among respondents currently in the active labor force and among those who are not. If the Stolper-Samuelson interpretation of the education effect is accurate, this effect should be contingent on whether or not individuals are actually being paid for the employment of their skills in the labor

⁷ On these points, see also Holsti 1996, 87---88.

⁸ For general discussions, see Wendt 1999, Ruggie 1998, and Finnemore and Sikkink 1998. For studies of strategy, see Katzenstein 1996; on human rights, see Sikkink 1993.

market.⁹

III. The Effects of Education on Trade Preferences

Our new empirical tests draw from two key sources of data on individual trade preferences: the U.S. National Election Studies (NES) and the International Social Survey Program (ISSP). These are the same datasets employed by the two most prominent studies of individual attitudes toward trade in recent years: the analyses by Scheve and Slaughter (2001a, 2001b) and Mayda and Rodrik (2005). In the following section we briefly describe each dataset and present the results for our split-sample tests.

A. Tests Using the NES Data

The NES surveys are fielded in the United States around the time of presidential elections and designed to gather data on Americans' social backgrounds, political predispositions, opinions on questions of public policy, and participation in political life.¹⁰ In 1992, for the first time, the NES included a question that asked respondents about their attitudes toward international trade. The question was the following:

Some people have suggested placing new limits on foreign imports in order to protect American jobs. Others say that such limits would raise consumer prices and hurt American exports. Do you favor or oppose placing new limits on imports, or haven't you thought much about this?

Scheve and Slaughter (2001a) used responses to this question from the 1992 NES survey as their measure of individual trade policy preferences. We have replicated their approach here in order to conduct the split-sample test, while also examining data from the 1996 NES survey that included

⁹ Similar split-sample tests have been used in studies of anti-immigrant sentiments to help discern whether greater opposition to immigration among less educated survey respondents (compared with more educated counterparts) reflects different degrees of concern about having to compete with immigrants in the job market: see Scheve and Slaughter 2001c; Mayda 2004; also Hainmueller and Hiscox 2004.

¹⁰ For details, see Miller et al. 1992; Rosenstone 1996. See also: <http://www.umich.edu/~nes/>

the same trade question – and which is also examined by Scheve and Slaughter (2001b). We created the dichotomous dependent variable *trade opinion*, coded as 1 for responses that favored protection (“new limits on foreign imports”) and zero for those opposed. The “haven’t thought much about this” answers are coded as missing, as in the Scheve and Slaughter analysis.

The principal measure of education is *schooling*, which simply records the years of full-time education completed by each respondent (a cap is set by the NES at 17 years). This is the measure employed by Scheve and Slaughter. To allow for nonlinear education effects, we have also constructed a set of dummy variables indicating each respondent’s highest level of educational attainment: *junior high* (1=8 years of schooling; 0=otherwise); *high school* (1=high school degree; 0=otherwise); *higher education* (1=some years of post high school education, including junior or community college; 0=otherwise); *college* (1=4-year bachelor’s degree; 0=otherwise); and *graduate* (1=postgraduate degree; 0=otherwise).¹¹ Assuming that education has linear effects on trade preferences seems appropriate if one assumes that each additional year education (of any type) improves an individual’s skills (and changes his or her attitudes) by a roughly constant amount. It is less appropriate if education has other, nonlinear types of effects associated with the ideas and information individuals possess about the way the economy works or the kinds of values and preferences they develop – as noted above, college education appears to play an overwhelming role in terms of its impact on ideas and cultural values among individuals.

For different sub-samples of respondents, we estimated binary probit models using *trade opinion* as the dependent variable, testing for the effects of *schooling* or the different education dummy variables while controlling for a variety of other individual characteristics that might

¹¹ Note that there are too few respondents who failed to finish elementary school to allow us to estimate the separate effect of elementary level education here: the excluded category in our analysis of education effects when using these dummy variables is all those with less than 8 years of schooling.

plausibly affect trade preferences. We estimated each model with two sets of covariates: a limited set of standard socio-demographic controls (*age*, in years, *gender*, and *race*), which preserved the maximum number of observations across the sub-samples; and a more extensive set of controls (the standard controls plus indicators of *union membership*, *party identification*, and *ideology*), which closely matched the more extensive specifications used by Scheve and Slaughter.¹² (See the appendix for a description of all variables used along with summary statistics).

¹² We have used *age* in years, since this is the most straightforward approach and is the measure provided in the NES data; Scheve and Slaughter (2001b) instead included a range of dummy variables covering separate age brackets. We have re-estimated all our results using the respective age dummies and the results are virtually identical. We have also used dummy variables for multiple racial categories here, while Scheve and Slaughter (2001b) appear to have used a single race dummy variable. Again, the results are almost identical regardless of how the race variable is entered in the models. The main variables we exclude are the ones constructed by Scheve and Slaughter using non-NES data: *sector net export share*, (net exports as a share of output for the industry in which the respondent is employed), and *sector tariff* (customs duties as a share of the total value of imports for the industry in which the respondent is employed). Neither of these variables have significant effects on trade preferences, according to the results reported by Scheve and Slaughter (2001b, 59-60), and their inclusion in the estimations makes no difference at all to the estimated effects of education on trade opinion. We also excluded the other variables Scheve and Slaughter derive from non-NES data – the measure of *occupation wage* (the national average weekly wage for the respondent’s occupation, which they use as an alternative to years of education as an indicator of skill level), and measures of *county exposure* to trade liberalization (the shares of total employment in the respondent’s home county accounted for by industries with above average tariffs or net imports). Scheve and Slaughter find that county exposure, when interacted with a dichotomous indicator of home ownership, does have a significant negative impact on support for trade openness, but the inclusion of these added controls in their models has a minuscule and statistically insignificant effect on the estimated impact of education on trade preferences (2001a, 285; 2001b, 64), so we have not attempted to replicate the construction of these added controls here. Note that, since there is very little missing data for

We expect the measures of education to be negatively associated with support for trade protection (as measured by *trade opinion*), either because highly skilled individuals expect trade to increase their real wages and poorly skilled respondents expect trade to decrease their real wages (a la Stolper-Samuelson), because more educated respondents know more about the overall economic benefits associated with trade openness, and/or because the more educated are less likely to nurture anti-foreigner sentiments. If this link between education and individual attitudes toward trade is primarily due to expectations about wages, however, the results from our estimations of trade preferences among respondents not actively engaged in the labor market should differ substantially when compared with those from our estimations of preferences among respondents who are currently employed. To test for this difference, we created sub-samples of the full NES survey sample, separating those who were in paid work from those not in paid work.¹³ Since those not currently in paid work include a varied set of individuals, such as those that are unemployed, students, and homemakers (and may be seeking paid work or plan to seek to

variables measured in the NES (the schooling measure has only 6% of observations missing in 1992, and 0% missing in 1996), and there is less missing data for all the other variables), we do not have to resort to imputation of missing data – Scheve and Slaughter (2001a, 278) report having to impute up to 73.4% of the observations for other (non-NES) variables they included in their analysis.

¹³ The sub-samples are determined by answers to a NES question asking respondents about their current employment status. Answer choices included: Working now, temporarily laid-off, unemployed, retired, student, homemaker, and permanently disabled. Our currently (not) in paid work sub-sample includes those that (did not) answered “in work now”. Those with missing employment status are coded as missing. We also include those very few retired/students/disabled/ homemakers who also indicated that they are “currently working more than 20 hours per week” in our currently-in-paid-work sub-sample. When the latter are excluded from the currently-in-paid sub-sample, the magnitudes of the schooling effect become, if anything, more similar across the in- and out-of-paid-work sub-samples than in the results we show here. Full results of these robustness tests are available upon request.

work soon), we have also isolated one particular group – those individuals who are retired – who are highly unlikely to re-enter paid work in the future and be concerned about how their (potential) wages might be affected by trade.¹⁴

The key results from the estimations are reported in Table 1, which displays the estimated effects of education on individual trade preferences in the full NES sample and in each of the different sub-samples. To facilitate comparison across sub-samples, rather than showing estimated probit coefficients, we report estimated marginal effects: that is, the change in the probability of favoring protectionism associated with an infinitesimal change in *schooling* (for the specific dummy variables for levels of highest educational attainment, the discrete change in the probability is shown).

[Table 1]

Comparing the results across the sub-samples we find little difference in the estimated effects of education on attitudes toward trade. In all cases, the estimated effects of *schooling* are very similar, both in terms of magnitude and level of statistical significance, across all models (none of the coefficients is significantly different from the others across sub-samples at conventional levels). This is true for estimations using both the 1992 and the 1996 NES data. For example, in the case of the 1996 survey, using the extensive set of covariates, a change from zero

¹⁴ While pensions for retired workers in some prominent U.S. industries (e.g., steel) have been linked to the financial health of their former employers, this is the exception and not the rule. Recent studies of U.S. retirees indicate that less than 17% of retirement income in the median household comes from employer-provided pension plans (see Sass 2003, 6; Social Security Administration 2002). And the connection between employer-provided pensions and the financial health of the firm is attenuated by the standards for funding and fiduciary conduct established by the Employee Retirement Income Security Act in 1974 (see Sass 1997). On this issue, we might also note that we get identical results when we perform the same tests comparing retirees with workers using the ISSP data (see below), drawn from a variety of countries with a variety of pension and retirement income systems.

to seventeen years of *schooling* (while holding the other covariates at their respective sample means) is associated with an average decrease in the probability of favoring protection of about 0.59 (s.e. 0.05) for the full sample, 0.51 (s.e. 0.09) for those currently in paid work, 0.48 (s.e. 0.11) for those currently not in paid work, and 0.53 (s.e. 0.11) for those who are retired (models 9-12, panel B).¹⁵ The observed relationship between education and trade preferences becomes even more similar across sub-samples once we replace the *schooling* measure with the separate education dummies. For example, compared to individuals with less than junior-high level educations, completing a *college* education decreases the probability of being in favor of protection by about 0.28 (s.e. 0.08) for the full sample, by 0.27 (s.e. 0.11) for those currently in paid work, by 0.31 (s.e. 0.13) for those not in paid work, and by 0.37 (s.e. 0.16) for retirees (see models 13-16, panel B).¹⁶

Overall, these results clearly indicate that the link between education and trade preferences reflects a causal mechanism that is not associated with concerns about the labor market and relative wage rates. The findings here are consistent with both types of alternative interpretations, however, suggesting that more educated individuals favor free trade because they are more likely to understand the economic case for trade openness and/or because they possess more cosmopolitan (or less xenophobic) outlooks – since there is no reason to believe that the effects of education on the economic knowledge or cultural values possessed by different individuals is conditional at all upon whether or not they are currently in the workforce.

These alternative interpretations are also supported by the clear nonlinearities that are apparent in the relationship between education and views about trade. Once we relax the linearity

¹⁵ Predicted effects here, and below, are calculated using the “Clarify” software developed by King, Tomz, and Wittenberg (2001). For each such calculation, all other covariates are set at the sample mean values.

¹⁶ Note that since we show marginal effects, the magnitudes of the effects of the dichotomous education variables can be read directly from the table.

restriction imposed by simply counting years of education (the *schooling* measure) we can see that the effects of education on trade preferences are described by a non-linear step function: *college* education has by far the greatest negative impact on support for protection, about three times larger than *higher education* (which is not itself a robust predictor), and about 6 times bigger than *high school* education (which is also not itself a robustly significant predictor and seems to have, if anything, a positive effect on support for protection) or completion of *junior high* education (which also enters mostly insignificant and with alternate signs); the impact of *graduate* education is almost identical to the effects of *college* education. For instance, looking again at the estimates in the 1996 NES (full sample), and the limited set of covariates, compared with individuals with less than a junior high education, completing *college* education reduces the probability of supporting protection by 0.31 (s.e. 0.08), compared to only 0.04 (s.e. 0.09) for *higher education*; finishing *high school* education *raises* the probability of support for protection by 0.04 (s.e. 0.08) and so does completing *junior high* grades by 0.11 (s.e. 0.14), though all three latter effects are not statistically significant. Meanwhile, completing *graduate* education reduces the probability of protectionist views by 0.35 (s.e. 0.08), an effect not significantly different from that associated with completing an undergraduate college education (see model 13, panel A).

There appears to be a very clear “plateau” effect here, with exposure to *college* education being the critical contributor to the generally positive relationship between education and support for free trade. This college plateau effect seems out of place with the story about labor market competition and its effects on trade preferences, especially since it applies to retirees as much as to those individuals currently active in the job market. On the other hand, this pattern fits much better with alternative approaches to explaining attitudes toward trade and the foreign affairs that focus on competing ideational and cultural perspectives and the way education – but especially college education – teaches students to think about trade and globalization in different ways or to evaluate it by different standards.

B. Tests Using the ISSP Data

Our second dataset is the 1995 National Identity module of the International Social Survey Programme (ISSP), the main data examined by Mayda and Rodrik (2005) in their recent study of the determinants of individual trade preferences. The ISSP collects cross-national data by coordinating and combining national surveys on a variety of topics: the 1995 module posed a variety of questions about national identity, patriotism, attachments to local communities, feelings about foreigners, and attitudes towards foreign trade and immigration.¹⁷ It provides information on some 28,500 respondents from 23 countries, including the United States, Canada, Japan, many Western and Eastern European countries, and one developing country (the Philippines).¹⁸

Our treatment of the ISSP data follows precisely the measurement and estimation strategies employed by Mayda and Rodrik (2005). Accordingly, we measure individual trade policy preferences using respondents' answers to the following question:¹⁹

Now we would like to ask a few questions about relations between (respondent's country) and other countries. How much do you agree or disagree with the following statements: (Respondent's country) should limit the import of foreign products in order to protect its national economy.

Options: 1 *Agree strongly*

¹⁷ For details, see ISSP 1995. See also: <http://www.issp.org/>

¹⁸ The specific countries are West Germany, East Germany, Great Britain, United States, Austria, Hungary, Italy, Ireland, Netherlands, Norway, Sweden, Czech Republic, Slovenia, Poland, Bulgaria, Russia, New Zealand, Canada, Philippines, Japan, Spain, Latvia, and Slovak Republic.

¹⁹ See Mayda and Rodrik (2005, 1401) for a discussion of the potential problems associated with the question wording.

2	<i>Agree</i>
3	<i>Neither agree nor disagree</i>
4	<i>Disagree</i>
5	<i>Disagree strongly</i>
8	<i>Can't choose, don't know</i>
9	<i>NA, refused</i>

Following Mayda and Rodrik, we created two binary variables, labeled *Pro-Trade-Dummy* and *Against-Trade-Dummy*, based upon responses to this question. *Pro-Trade-Dummy* is coded 1 for individuals opposing trade protection (i.e. for those replying “disagree” or “disagree strongly” to the question), and zero otherwise. *Against-Trade-Dummy* is coded 1 for individuals favoring trade protection (i.e. for those replying “agree” or “agree strongly” to the question), and zero otherwise. Accordingly, the “Can’t choose, don’t know” and “NA, refused” responses are coded as zero. Since the results from the estimations of the *Pro-Trade-Dummy* and *Against-Trade-Dummy* are substantively very similar, we just emulate Mayda and Rodrik here and present only the results using the analysis of the *Pro-Trade-Dummy*.²⁰

Once again the principal measure of education, *schooling*, is simply a count of the years of full-time education completed by each respondent (we follow Mayda and Rodrik and set a cap at 20 years). In order to test for nonlinearities in the effects of education on attitudes, we again construct a set of highest educational attainment dummy variables which include *secondary incomplete* (1=some secondary education; 0 otherwise), *high school* (1=high school degree; 0=otherwise), and *college* (1=university education; 0=otherwise). The reference category here comprises those respondents who have only a primary school education (or less). Unlike the NES

²⁰ Mayda and Rodrik note that their results are also unchanged if they treated “Can’t choose, don’t know” and “NA, refused” as missing data, and the same is true for our findings. In addition, Mayda and Rodrik report that they also used a categorical dependent variable (labeled TRADE_OP) to estimate ordered logit (and OLS regression) models and obtained “very similar” substantive results (2005, 1397).

data, the ISSP does not allow us to distinguish any finer categories here; no separate coding for graduate education is available, so the college category here includes respondents with advanced or higher university degrees (and those who attended college but did not complete a degree).

Our estimation strategy is identical to that one we applied for the analysis of the NES data. We estimate two series of binary probit models in which the *Pro-Trade Dummy* is estimated using either *schooling* or the set of education dummy variables and other covariates. We estimated all the models for all sub-samples using three sets of controls. Our limited set of controls includes just *age*, *gender*, and *citizenship* and a full set of country dummies. This mirrors precisely the baseline specification as presented in Mayda and Rodrik (2005, p. 1403, Table 4, column 1). Our extensive set of controls matches those included in their “demographic model” (Table 4, column 2) and includes *age*, *gender*, *citizenship*, *area of residence* (i.e., rural, suburban, or urban residence), *subjective social class*, *political party affiliation*, *trade union membership*, the log of *real income* and a full set of country dummies.²¹ Our third model mirrors Mayda and Rodrik’s “factor endowments model” (their Table 4, column 3). It includes the covariates *age*, *gender*, *citizenship*, and a full set of country dummies, as well as a *schooling*gdp* multiplicative term that interacts the schooling measure with the log of GDP per capita in the respondent’s country alongside the main effect of *schooling*.²² The idea here is to capture potential cross-country heterogeneity with respect to the education effect. GDP per capita is used here as a simple indicator of relative factor abundance; the assumption is that higher levels of GDP per

²¹ Following Mayda and Rodrik (2005) we compute real income using data in local currency from the ISSP dataset and purchasing power parity conversion factors taken from the World Bank’s *World Development Indicators* database. See <http://www.worldbank.org/data/wdi2004/>

²² GDP per capita data is taken from the WDI (1995 current international dollars, adjusted for purchasing power parity). Note that the direct effect of country GDP per capita on the attitudes of respondents is captured by the country dummies and is not estimated separately. See Mayda and Rodrik (2005, 1405) for a detailed discussion of the specification.

capita are associated with a greater abundance of skilled labor. According to the Stolper-Samuelson theorem, skilled individuals will gain in terms of real wages, and thus should be likely to favor trade openness, in countries that are abundantly endowed with skilled labor, but the size of those gains should be proportional to the degree of skill abundance in each country (and, of course, in countries that are actually poorly endowed with skilled labor relative to potential trading partners, those gains may become losses). In terms of the anticipated results this means that the *schooling*gdp* interaction term should be positive. In every model we calculate robust standard errors allowing for potential within-country clustering. (See the appendix for descriptions of all variables and summary statistics).

Again we created sub-samples of the full ISSP survey sample, separating those who were in paid work from those not in paid work. Since the ISSP variable coding the employment status of respondents also differentiates between those in full and part-time employment, we define those in (and not in) paid work using both possible combinations (just those in full-time work or all those working).²³ And again, we have estimated the models separately for those individuals who are retired. We estimated all models for the full sample first, and then for each particular sub-sample. The results are reported in Table 2, which displays just the estimated marginal effects of education on trade preferences.

[Table 2]

²³ The sub-samples are determined by answers to a question asking respondents about their current employment status. Answer choices included: full-time employed, part-time employed, less than part-time employed, helping family member, unemployed, student, homemaker, retired, permanently disabled, and other. Our currently-in(not-in)-paid-work sub-sample includes all those (not) answering “employed (either part-time or full time)” to this question. Those with missing employment status are coded as missing. We also re-estimated all models including those working “less than part-time” in the currently-in-paid-work sub-samples. The results are substantively identical to those in Table 2 (and are available upon request).

Overall, the findings from the analysis of the ISSP data are very similar to the results from the analysis of the NES data reported above. Here the estimated effects of *schooling* on attitudes towards trade are almost identical, both in terms of magnitude and level of statistical significance, across all sub-samples, and regardless of whether we examine the baseline (panel A), the demographic (panel B), or the factor endowment models (panel C). None of the schooling effects are significantly different from others across the sub-samples at conventional levels. For example, in the baseline model, a shift from zero to twenty years of *schooling* (while holding the other covariates at their respective sample means) increases the probability of favoring trade by about 0.34 (s.e. 0.05) in the full sample, by 0.37 (s.e. 0.06) in the in (full-time) work sub-sample, by 0.31 (s.e. 0.05) for the not-in-full-time-work sub-sample, and by 0.33 (s.e. 0.05) in the retired sub-sample (models 1-6, panel A). These estimated effects of education are very similar regardless of whether those working on a part-time basis are included in the “paid work” sub-sample or not. The same holds true if we account for potential cross-country heterogeneity with respect to the education effect (panel C). Both the direct effect of schooling and its interaction with GDP per capita are almost identical across sub-samples. The similarity in these effects across the different sub-samples of respondents is again clearly at odds with the claim that the *schooling* coefficients are primarily indicative of concerns among respondents about the effects of trade openness on labor markets and relative wages.

If we relax the linearity restriction and replace the *schooling* measure with the education dummy variables, it is clear that the impact of the education on trade preferences remains very similar across the sub-samples. It also becomes apparent again that the effects of education on opinions about trade are non-linear. Specifically, *college* education has by far the greatest positive effect on individual-level support for free trade, two-to-three times bigger than the impact of *high school* education (which is not a robustly significant predictor of attitudes); meanwhile, *incomplete secondary* education appears to have little if any effect on support for free trade (only very few of the coefficients enter significant at conventional levels and some even switch signs).

For instance, in the demographic model (model 7, panel B), and comparing with individuals with only primary level education, *college* education shifts the probability of being pro-trade by 0.14 (s.e. 0.04) as compared to only 0.06 (s.e. 0.04) for completing *high school* education; and *incomplete secondary* education increases the probability of being pro-free trade by a mere 0.02 (s.e. 0.04) – the latter two effects are insignificant at conventional levels. The same non-linearities are clear when cross-country heterogeneity in the education effect is taken into account. For example, for the full sample in the factor endowments model (model 7, panel C), evaluated at the sample mean level of GDP per capita among the ISSP nations (\$15,987), a *college* education raises the probability of being pro-trade by 0.18 (s.e. 0.01) compared to a primary level education; the corresponding effect is only 0.09 (s.e. 0.01) for completing *high school* education, and an *incomplete secondary* education increases the probability of being pro-free trade by a mere 0.04 (s.e. 0.01). The pattern here strongly resembles the college “plateau” effect found in the analysis of the NES data, and again confirms – this time based upon data drawn from surveyed 23 nations – that exposure to *college* education among individuals lies at the heart of the observable relationship between education and support for free trade. Regardless of which model we estimate, the college effect is strikingly similar across all sub-samples.

One issue we need to address here concerns the cross-country heterogeneity with respect to the education effect that is evident in the results from the estimations of factor endowments model. As reported by Mayda and Rodrik, we find that the *schooling*gdp* interaction term is positive – in all of the sub-samples. The implication is that the impact of education on individual attitudes toward trade varies by country depending on levels of economic development. For example, examining the full-sample estimates (model 1, panel C), a shift from zero to twenty years of *schooling* (other covariates at their respective sample means) increases the probability of favoring trade openness by about 0.49 (s.e. 0.03) in a country with GDP per capita of \$22,039 (i.e., Germany in 1995) but by only 0.27 (s.e. 0.02) in a country with GDP per capita of \$11,720 (the Czech Republic in 1995).

This result is consistent with the Stolper-Samuelson theorem, but it also seems very consistent with the alternative ideational and cultural accounts of the causal effects of education on attitudes toward trade (accounts that can also explain the similarity in the effects of education across labor-market sub-samples in each country). In line with an ideational approach, it seems highly likely that there are important cross-national differences in the content and quality of instruction in economics at the college level that are related to cross-national differences in levels of development across these surveyed countries. Economists and other scholars in less developed countries may be more concerned than their counterparts in wealthier nations about the adjustment costs associated with trade liberalization, or the fairness of trade negotiations and agreements, and convey these concerns to students. Or, more in line with a cultural account that stresses the link between education and values, it also seems very plausible that education systems in richer ISSP nations, which have longer histories of democratic institutions than poorer nations, may place greater emphasis on inculcating tolerance among students, and thus may more effectively discourage anti-foreigner (and protectionist) sentiments. The general point here is that there are plausible explanations for the observed differences between richer and poorer ISSP countries in the effects of education on attitudes toward trade that fit with the ideational and cultural accounts; the similarities in the effects of education across sub-samples of individuals *within* each country, and the college plateau effect, are also consistent with these same accounts, but they contradict the Stolper-Samuelson view.²⁴

²⁴ In general we do not place great importance on the finding that the *schooling*gdp* interaction term is positive for the ISSP set of countries – in our analysis of the alternative Pew data on individual trade preferences, which includes a much larger and more varied set of countries (see discussion below), we do not find the same, simple positive relationship between GDP per capita and the magnitude of the education effect. The breakdown of this relationship among countries with more varied political and institutional histories suggests that it does not provide firm support for a Stolper-Samuelson interpretation. These results are available at: http://www.people.fas.harvard.edu/~hiscox/HainmuellerHiscox_IOa.html

C. Family Income Effects?

It seems impossible to square the findings we have reported above with a simple Stolper-Samuelson reading of the effects of education on trade policy preferences. One possible counter-argument, in defense of the Stolper-Samuelson interpretation, is that individuals who themselves are not employed or seeking employment are nevertheless concerned about the distributional effects of trade because it will have an impact on their total *family* or household income. Thus, individuals who are not employed and describe themselves as “homemakers” may actually be making calculations about the effects of trade on the earnings of their spouse or partner when responding to survey questions. Similarly, retired individuals may be taking into account the effects of trade on the earnings of their children. If these are powerful concerns, and if there is a strong and positive within-family correlation in levels of education, expectations about within-family income transfers might feasibly explain the results we are reporting here in a way that would still be broadly consistent with the spirit of the Stolper-Samuelson theorem. To the best of our knowledge, this family income extension of the theorem has not actually been advanced or tested before – no such claims are made by Scheve and Slaughter (2001a, 2001b) or Mayda and Rodrik (2005). On the face of it, we think this potential explanation seems quite implausible, given the clear similarities between the education effects across sub-samples, the imperfect correlation between individual education attainment and spouses’ or partners’ (or children’s) education levels, and the uncertainty attached to within-family income transfers. The connection between retirees’ incomes and the earnings of their children seems especially tenuous, since most retirees in western countries rely overwhelmingly upon personal savings, retirement accounts, and state-provided pensions for their retirement income.

We tested the alternative explanation using the NES data, which provide the best available data on individual marital status.²⁵ The NES includes the following question: “Are you married now and living with your (husband/wife) – or are you widowed, divorced, separated, or have you never married?” Following the NES recommendations, we created a dummy variable *partner*, coded 1 if the respondent indicated he or she is currently living with a spouse or partner and 0 otherwise. To provide an explicit test of spouse and partner effects, we simply repeated all our split-sample analysis using the *partner* variable to further divide each labor market sub-sample according to marital status.²⁶ Dividing the sample in one more dimension results in a limited number of observations within each sub-sample, of course, so we estimate the model with only the limited set of covariates (including *age*, *gender*, and *race*) here, and we use only the 1992 NES data (the 1996 NES data contains half the total number of observations of the 1992 NES sample, as noted above).²⁷ Results from these estimations are reported in Table 3. Again we report estimated marginal effects for the education variables so that magnitudes are directly comparable across sub-samples.

[Table 3]

²⁵ Unfortunately, we cannot conduct a similar analysis for the ISSP survey, because far too much data is missing on the marital status of the respondents. We have repeated these tests using the data from the GAP survey, however, and have found substantively identical results (findings from these additional tests are also available from the authors).

²⁶ We have also re-run our models for each sub-sample using a multiplicative term (*partner*schooling*) alongside the main effect of *partner* and the results are very similar to the ones reported below. Nonetheless, given the high correlation between the multiplicative and its lower order terms we prefer the split-sample tests as our identification strategy.

²⁷ Note too, that in the specifications that replace the single schooling variable with separate educational dummies, we also exclude the *junior high* attainment dummy, since there are no observations for this category in some sub-samples.

The similarities in the effects of education across sub-samples appear again here, regardless of whether we limit the analysis to those respondents who have a spouse/partner or to those who do not. In all cases the estimated effects of *schooling* are very similar, both in terms of magnitude and level of statistical significance. Most importantly, within *and* across each sub-sample, we cannot reject the null hypothesis that the education effects are identical (at the 90% percent level). For example, for the full sample estimates, a change from zero to seventeen years of *schooling* (while holding the other covariates at their respective sample means) is associated with an average decrease in the probability of favoring protection of about 0.48 (s.e. 0.03) for all respondents, 0.50 (s.e. 0.04) for those respondents that have a partner/spouse, and 0.44 (s.e. 0.06) for those that do not have a partner/spouse. Looking only at those that are currently in paid work, the respective decrease is 0.50 (s.e. 0.04) for all respondents, 0.51 (s.e. 0.05) for those that have a partner/spouse, and 0.47 (s.e. 0.08) for those that do not. And for those respondents that are out of the workforce, the respective decrease is 0.35 (s.e. 0.07) for all, 0.38 (s.e. 0.09) for with a partner, and 0.35 (s.e. 0.11) for those without. Finally, for the retired sub-sample, the respective decrease is again very similar, about 0.32 (s.e. 0.11) for all, 0.33 (s.e. 0.14) for those with and 0.33 (s.e. 0.18) for those without a partner/spouse.

The same results emerge when we re-run the models, replacing the *schooling* measure with the *educational attainment* dummies. Most importantly, the key *college* effect remains strikingly stable across models even if we divide the sub-samples using the *partner* variable. For example for the full sample, completing college education decreases in the probability of favoring protection by about 0.30 (s.e. 0.05) for all respondents, by about 0.30 (s.e. 0.06) for those with partner/spouse, and by 0.34 (s.e. 0.08) for those without (all compared to those that did not complete a high school degree). For those currently in work, the respective decrease is 0.29 (s.e.0.07) for all respondents, 0.27 (s.e. 0.08) for those with, and 0.33 (0.14) for those without a partner/spouse. For those not working, the respective decrease is 0.32 (s.e. 0.09) for all respondents, 0.39 (s.e. 0.13) for those with, and 0.44 (s.e. 0.12) for those without a

partner/spouse. The retired sub-sample reveals the greatest differences in magnitudes of the *college* effect. The decrease in pro-protection attitudes associated with completing *college* is 0.41 (s.e. 0.12) for all respondents, 0.28 (s.e. 0.17) for those with, and 0.66 (s.e. 0.10) without a partner. This difference is, if anything, the *opposite* of what one would expect if one simply assumed that retirees with partners were assessing the effects of trade on the incomes of their (working and similarly educated) partners while retirees without partners were not.

If we just compare the effects of *college* (or *schooling*) across sub-samples of respondents that do *not* have partners – a test that presumably excludes the bulk of any family income effects – it is clear that these effects are not significantly different, regardless of whether those respondents are working, not working, or retired. Certainly the effects are not significantly larger for those being paid for the employment of their skills than for others, as implied by a simple Stolper-Samuelson interpretation of the impact of education. Overall, the findings suggest that a broader family or household interpretation of the Stolper-Samuelson theorem cannot account for the results we have found.

IV. Findings, Implications, and Future Research

Previous research has concluded that the simple association that is evident between education and support for trade openness among survey respondents in the United States and other Western nations is proof positive of the Stolper-Samuelson theorem. That is, this relationship is seen as a straightforward confirmation that high-skilled individuals in skill-abundant economies perceive that trade openness raises their real wages while low-skilled individuals calculate that their real wages decrease with trade liberalization. But if these calculations are paramount, the effects of education on individual trade policy preferences should be conditional upon whether or not individuals are actually being paid for the employment of their skills in the labor market. Re-examining the available survey data on individual attitudes toward international trade in a variety of nations, we find that this is simply not the case. The impact of education on attitudes toward trade is almost identical among respondents currently in

the active labor force and among those who are not – and even among those who are retired from work. And the impact of education on attitudes is overwhelmingly due to college education. We have replicated all the tests we report above using two alternative cross-national survey datasets – the third wave of the World Values Survey carried out in 1995-1997, and the Global Attitudes Project survey administered by Pew in 2003 – and we have found substantively identical results.²⁸

Given these results, a fundamental reinterpretation of the survey evidence on attitudes toward trade and globalization appears to be in order. The results reported above indicate that the effects of education levels on trade policy preferences are *not* primarily a reflection of distributional concerns among survey respondents. We suggest instead that the impact of education on how individuals think about trade and globalization has more to do with ideas and information about the aggregate effects of these economic phenomena. Some part of the effect of education on attitudes toward trade might also reflect the fact that college-educated individuals are less likely than others to harbor anti-foreigner sentiments. Which of these two causal mechanisms – ideational or cultural – is more important? Unfortunately, no opinion surveys (of which we are aware) gather all the data that one would need to answer this question definitively. That is, no surveys ask respondents about their views regarding trade, also ask questions designed to measure economic knowledge or exposure to economic ideas, and also ask questions that would gauge individuals' commitments to values such as tolerance and cosmopolitanism. For now we only have some clues provided by studies of partial sets of data – but these clues suggest to us that the smart betting would be on the ideational argument.

One major clue comes from the available data on economic knowledge among the public and its impact upon trade policy preferences. As far as we are aware, the only survey that has

²⁸ Full results from all these separate tests are available in a supplement to this paper that can be downloaded at: http://www.people.fas.harvard.edu/~hiscox/HainmuellerHiscox_IOa.html

actually combined measures of both economic knowledge and attitudes toward trade is the 1992 poll conducted by William Walstad and Max Larsen for the National Center for Research on Economics Education and for Gallup (see Walstad 1997; Walstad and Rebeck 2002). To gauge “economic literacy” – the main purpose of the study – the survey asked respondents a battery of substantive economic knowledge questions. What is especially interesting, in light of the discussion above, is that once this measure of specialized economic knowledge is included in estimations of individual support for trade openness, the effects of the standard measures of education on attitudes toward trade shrink dramatically in magnitude and none of these effects remains statistically significant (see Walstad 1997, 201. That is, economic literacy appears to account for most of the “raw” impact of education on attitudes about trade.²⁹ And this literacy effect is very stable across various sub-samples of respondents who are in the active labor force and those who are not.

On the other side of this coin, adding various measures of individuals’ commitments to various relevant cultural values (like tolerance and cosmopolitanism) to estimations of attitudes toward trade appears to have almost no affect at all on the magnitude or significance of the effects of standard measures of education. For instance, using the ISSP survey data that asked a large number of “identity” questions, Mayda and Rodrik (2005, 1414---6) introduced an array of controls for (self-expressed) national pride and attachment to local their communities when estimating individual support for trade protection. Many of these cultural variables had significant and sizeable effects on trade preferences, but they had no significant effect at all on the estimated effects of education on such preferences. Using a variety of different measures of racial and cultural tolerance available in the 1992 NES survey, we have found very similar results – that is, while the various measures of tolerance among the respondents are themselves significant

²⁹ Full results from our own tests using the Walstad and Larsen data are available online at:

http://www.people.fas.harvard.edu/~hiscox/HainmuellerHiscox_IOa.html

predictors of views about trade, they account for none of the raw impact of education on trade policy preferences.³⁰

Finally, it is important to note that while these results indicate that the impact of education on trade preferences has more to do with ideas and economic literacy than it does with calculations about how trade affects personal income or job security, this is *not* to say that the latter types of calculations are not important in shaping individuals' views of trade. Our conclusion here is just that the simple association between education and support for free trade among surveyed individuals is not a clear manifestation of such distributional concerns. We strongly suspect that concerns about the effects of trade on personal economic well-being, income and security do play a large role in shaping trade policy preferences, but they are related much more directly to the impact that trade openness is expected to have in particular industries and firms. "Specific factors" models of the distributional effects of trade that, unlike the Stolper-Samuelson theorem, allow that factors of production are not perfectly mobile between different sectors in the economy, predict that the real incomes of individuals are tied closely to the fortunes of the particular industries in which they are employed or invested (see Jones 1971; Mussa 1974).³¹ To date, studies of individual trade preferences have indicated only weak (Mayda and Rodrik) or no (Scheve and Slaughter) support for the prediction that individuals employed in import-competing industries will be much less likely to support trade openness than those employed in exporting industries. But these studies have measured the industry-specific effects of trade in a very indirect way, locating respondents by industry using answers to a standard question about the type of business in which they are employed, then controlling for the aggregate

³⁰ Results from all these separate tests are available from the authors and can also be downloaded at: http://www.people.fas.harvard.edu/~hiscox/HainmuellerHiscox_IOa.html

³¹ The specific factors approach underpins much of the most recent analysis of the political economy of trade in contemporary advanced economies: see Magee 1980; Grossman and Helpman 1994; Hiscox 2002.

trade positions of those industries (e.g., their degrees of import penetration) when estimating individual trade preferences.³² There are at least two major problems with this approach: first, accurately coding respondents by industry of employment using standard industrial classifications is extremely difficult, as respondents typically give very vague answers to questions about the type of business in which they work;³³ second, aggregate industry measures of import penetration and export dependence may offer more information about policy outcomes than policy preferences and they obscure the obvious variation in positions taken by firms in the sub-categories within each broad industry grouping.³⁴

One of the implications of our findings is that future survey-based measurements need to be substantially refined to better account for industry-specific effects if we are to accurately gauge the impact of distributional concerns on attitudes towards trade and globalization.³⁵

³² Scheve and Slaughter (2001a, 2001b) examine industry effects using this approach and industry coding supplied in the NES. Mayda and Rodrik (2005) were forced to take an extra step: since the ISSP data does not provide coding by industry, they inferred industry of employment from occupational codes assigned to respondents instead.

³³ When the staff at the Panel Study of Income Dynamics checked a random sample of surveys, for instance, they found that industry codes differed across coders in 14% of cases (see PSID 1999).

³⁴ The standard concern about using import penetration as a measure of an industry's trade policy preference is that low levels of penetration may reflect the effectiveness of a very protectionist lobby (not the absence of concerns about import competition).

³⁵ One recent study, for instance, questioned respondents directly about the likely impact of trade on the security of their own particular job, asking them whether they felt that increasing trade made their own job more secure, less secure, or had no clear effect (see Hiscox 2004). The estimated probability of support for trade was some 45% higher among respondents for whom trade had raised job security compared with those reporting that trade made their own job less secure.

Resolving these issues is crucial for improving our understanding of the determinants of anti-globalization sentiments and the range of policies that might address them.

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Table 1: The Effect of Education on Trade Preferences across Sub-Samples – NES Data

Survey	NES 1992								NES 1996							
DV	<i>Trade_Opinion (1= Favor Protectionism; 0=Otherwise)</i>								<i>Trade_Opinion (1= Favor Protectionism; 0=Otherwise)</i>							
Mean DV	0.67								0.52							
SD DV	0.46								0.49							
Sub-sample	<i>Full Sample</i>	<i>Currently in Paid-Work</i>	<i>Currently not in Paid-Work</i>	<i>Retired</i>	<i>Full Sample</i>	<i>Currently in Paid-Work</i>	<i>Currently not in Paid-Work</i>	<i>Retired</i>	<i>Full Sample</i>	<i>Currently in Paid-Work</i>	<i>Currently not in Paid-Work</i>	<i>Retired</i>	<i>Full Sample</i>	<i>Currently in Paid-Work</i>	<i>Currently not in Paid-Work</i>	<i>Retired</i>
Model No. ¹	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
PANEL A: Limited Set of Covariates²																
Schooling	-0.048*** (0.006)	-0.056*** (0.008)	-0.031*** (0.008)	-0.028** (0.011)					-0.058*** (0.010)	-0.058*** (0.012)	-0.044*** (0.016)	-0.044** (0.017)				
Junior High					-0.024 (0.087)	0.028 (0.154)	-0.056 (0.100)	-0.029 (0.131)					0.108 (0.139)	0.073 (0.218)	0.118 (0.134)	0.204** (0.103)
High School					0.028 (0.047)	0.051 (0.070)	0.016 (0.058)	0.070 (0.091)					0.036 (0.083)	0.007 (0.128)	0.065 (0.095)	0.042 (0.110)
Higher Education					-0.109** (0.051)	-0.113 (0.074)	-0.079 (0.068)	-0.072 (0.122)					-0.035 (0.085)	-0.022 (0.127)	-0.079 (0.109)	-0.068 (0.138)
College					-0.317*** (0.053)	-0.289*** (0.073)	-0.355*** (0.098)	-0.398*** (0.132)					-0.307*** (0.078)	-0.282** (0.112)	-0.353*** (0.119)	-0.372** (0.149)
Graduate					-0.315*** (0.061)	-0.305*** (0.081)	-0.259** (0.113)	-0.134 (0.149)					-0.348*** (0.075)	-0.315*** (0.102)	-0.365** (0.163)	-0.297 (0.197)
Observations	1604	1048	556	227	1563	1021	542	217	843	560	283	167	843	560	283	167
Log likelihood	-962.64	-646.24	-308.66	-128.20	-922.08	-621.46	-293.76	-114.85	-533.13	-360.69	-164.87	-89.61	-519.93	-353.56	-158.73	-86.96
Pseudo R-squared	0.05	0.06	0.04	0.06	0.07	0.07	0.05	0.11	0.09	0.07	0.09	0.14	0.11	0.09	0.12	0.16
PANEL B: Extensive Set of Covariates³																
Schooling	-0.049*** (0.006)	-0.057*** (0.008)	-0.032*** (0.008)	-0.025** (0.012)					-0.057*** (0.010)	-0.054*** (0.012)	-0.047*** (0.016)	-0.056*** (0.017)				
Junior High					-0.026 (0.098)	0.010 (0.165)	-0.076 (0.122)	-0.003 (0.131)					0.096 (0.152)	-0.017 (0.237)	0.144 (0.128)	0.196* (0.103)
High School					0.041 (0.050)	0.072 (0.078)	0.028 (0.062)	0.099 (0.092)					0.046 (0.086)	-0.007 (0.128)	0.083 (0.099)	0.031 (0.117)
Higher Education					-0.097* (0.055)	-0.098 (0.083)	-0.057 (0.069)	-0.070 (0.124)					-0.015 (0.089)	-0.033 (0.128)	-0.034 (0.114)	-0.068 (0.145)
College					-0.298*** (0.058)	-0.260*** (0.083)	-0.342*** (0.103)	-0.347** (0.142)					-0.276*** (0.083)	-0.273** (0.113)	-0.307** (0.131)	-0.367** (0.161)
Graduate					-0.301*** (0.065)	-0.286*** (0.090)	-0.234** (0.115)	-0.071 (0.146)					-0.329*** (0.080)	-0.316*** (0.103)	-0.303* (0.182)	-0.261 (0.214)
Observations	1501	989	512	213	1463	964	499	203	815	537	278	166	815	537	278	166
Log likelihood	-892.72	-604.92	-277.42	-117.09	-856.56	-581.61	-264.24	-104.87	-508.42	-341.85	-158.43	-85.46	-498.29	-335.93	-154.19	-85.23
Pseudo R-squared	0.07	0.07	0.06	0.09	0.08	0.09	0.07	0.13	0.10	0.08	0.11	0.17	0.12	0.09	0.13	0.17

1. Probit estimations: coefficients are estimated marginal effects ($\partial F/\partial x_k$), i.e. the marginal effect on $\Pr(y=1)$, given a unit increase in the value of the relevant (continuous) regressor (x_k), holding all other regressors at their respective sample means. The discrete change in the probability is reported for binary regressors. Robust standard errors in parentheses. * $p<0.10$ ** $p<0.05$ *** $p<0.01$. Each model includes a full set of controls of the respective covariates set (coefficients not shown here). Cases weighted by the respective NES sample weight (v923008 or v960003).

2. Limited set of covariates includes age, gender, and race.

3. Extensive set of covariates includes age, gender, race, union membership, party identification, and ideology. For details of variables see Scheve and Slaughter 2001a/b.

Table 2: The Effect of Education on Trade Preferences across Sub-samples – ISSP Data

Survey	ISSP 1995											
DV	<i>Pro-Trade-Dummy (1= Pro-Trade; 0 otherwise)</i>											
Mean DV	0.22											
SD DV	0.41											
Sub-Sample: (FT Full-Time; PT Part Time)	<i>Full Sample</i>	<i>in work (FT)</i>	<i>not in Work (FT)</i>	<i>in work (FT & PT)</i>	<i>not in work (FT&PT)</i>	<i>Retired</i>	<i>Full Sample</i>	<i>in work (FT)</i>	<i>not in Work (FT)</i>	<i>in work (FT & PT)</i>	<i>not in work (FT&PT)</i>	<i>Retired</i>
Model No.	1	2	3	4	5	6	7	8	9	10	11	12
PANEL A: Baseline Specification²												
Schooling	0.017*** (0.004)	0.019*** (0.005)	0.014*** (0.003)	0.019*** (0.004)	0.013*** (0.003)	0.016*** (0.002)						
Secondary Incomplete							0.035 (0.026)	-0.003 (0.042)	0.061*** (0.018)	0.008 (0.041)	0.056*** (0.017)	0.073*** (0.021)
High School							0.091** (0.036)	0.076 (0.051)	0.090*** (0.028)	0.085* (0.049)	0.082*** (0.028)	0.120*** (0.021)
College							0.186*** (0.045)	0.167*** (0.058)	0.183*** (0.038)	0.181*** (0.056)	0.165*** (0.041)	0.192*** (0.033)
Observations	26485	12205	13915	14279	11841	4932	28714	12997	15324	15182	13139	5445
Log likelihood	-12632.58	-6366.44	-6087.04	-7396.50	-5070.04	-1882.22	-13617.95	-6730.05	-6691.31	-7806.00	-5628.82	-1998.99
Pseudo R-squared	0.07	0.06	0.07	0.06	0.07	0.09	0.07	0.06	0.08	0.06	0.07	0.10
PANEL B: The Demographic Model³												
Schooling	0.015*** (0.003)	0.013*** (0.004)	0.015*** (0.003)	0.013*** (0.003)	0.018*** (0.003)	0.020*** (0.005)						
Secondary Incomplete							0.019 (0.036)	-0.023 (0.040)	0.070 (0.044)	0.008 (0.052)	0.041 (0.029)	0.018 (0.041)
High School							0.064 (0.039)	0.022 (0.048)	0.101*** (0.034)	0.035 (0.055)	0.115*** (0.026)	0.087*** (0.031)
College							0.137*** (0.043)	0.089* (0.047)	0.185*** (0.044)	0.108* (0.055)	0.187*** (0.045)	0.162** (0.078)
Observations	5809	3785	1934	4427	1290	503	6547	4138	2313	4834	1615	688
Log likelihood	-2805.44	-1946.62	-815.17	-2241.61	-521.34	-185.57	-3123.02	-2102.24	-968.15	-2421.76	-651.50	-230.39
Pseudo R-squared	0.09	0.09	0.11	0.09	0.13	0.16	0.10	0.09	0.11	0.09	0.13	0.15
PANEL C: The Factor Endowments Model⁴												
Schooling	-0.113*** (0.021)	-0.121*** (0.019)	-0.106*** (0.025)	-0.117*** (0.019)	-0.107*** (0.025)	-0.121*** (0.025)						
Ln(GDP)*Schooling	0.014*** (0.002)	0.015*** (0.002)	0.013*** (0.003)	0.014*** (0.002)	0.013*** (0.003)	0.014*** (0.003)						
Secondary Incomplete							-0.360*** (0.049)	-0.503*** (0.041)	-0.223*** (0.079)	-0.502*** (0.046)	-0.222*** (0.071)	-0.193** (0.081)
Ln(GDP)*Sec. Incomplete							0.072*** (0.016)	0.110*** (0.015)	0.040** (0.019)	0.111*** (0.017)	0.040** (0.017)	0.040* (0.023)
High School							-0.497*** (0.115)	-0.624*** (0.079)	-0.393*** (0.133)	-0.623*** (0.084)	-0.383*** (0.122)	-0.275*** (0.103)
Ln(GDP)*High School							0.095*** (0.027)	0.116*** (0.021)	0.077** (0.031)	0.116*** (0.022)	0.077*** (0.029)	0.075** (0.033)
College							-0.551*** (0.093)	-0.710*** (0.064)	-0.425*** (0.112)	-0.705*** (0.071)	-0.398*** (0.094)	-0.322*** (0.064)
Ln(GDP)*College							0.144*** (0.029)	0.164*** (0.022)	0.132*** (0.040)	0.161*** (0.023)	0.137*** (0.038)	0.132*** (0.028)
Observations	26485	12205	13915	14279	11841	4932	28714	12997	15324	15182	13139	5445
Log likelihood	-12543.62	-6325.67	-6042.16	-7353.64	-5027.18	-1861.63	-13526.51	-6683.06	-6647.11	-7754.93	-5584.56	-1981.49
Pseudo R-squared	0.08	0.07	0.08	0.07	0.07	0.10	0.08	0.07	0.08	0.07	0.08	0.11

1. Probit estimations: coefficients are estimated marginal effects ($\partial F/\partial x_k$), i.e. the marginal effect on $\Pr(y=1)$, given a unit increase in the value of the relevant (continuous) regressor (x_k), holding all other regressors at their respective sample means. The discrete change in the probability is reported for binary regressors. Robust standard errors in parentheses. * $p<0.10$ ** $p<0.05$ *** $p<0.01$. Each model includes a full set of controls of the respective covariates set (coefficients not shown here). Cases weighted by ISSP sample weight (v342).

2. Baseline Specification: Covariates includes age, gender, citizenship, and country dummies (this set of covariates is equal to Mayda and Rodrik 2005 Table 4, column 1).

3. The Demographic Model Specification: Covariates includes age, gender, citizenship, subjective social class, log of real income, rural, union membership, partisan orientation, and country dummies (this set of covariates is equal to Mayda and Rodrik 2005 Table 4, column 2).

4. The Factor Endowment Model Specification: Covariates includes age, gender, citizenship, schooling, a multiplicative term $\ln(\text{GDP per capita 1995, PPP}) \times \text{schooling}$, and country dummies (this set of covariates is equal to Mayda and Rodrik 2005 Table 4, column 3). Note that the country dummies pick up the main effect of $\ln(\text{GDP})$.

Table 3: The Effect of Education on Trade Preferences across Labor Market and Partner Sub-Samples

Survey	NES 1992											
DV	Trade_Opinion (1= Favor Protectionism, 0=Otherwise)											
Mean DV	0.67											
SD DV	0.46											
Sub-sample 1	Full Sample			Currently in Paid-Work			Currently not in Paid-Work			Retired		
Sub-sample 2	All	Partner	No Partner	All	Partner	No Partner	All	Partner	No Partner	All	Partner	No Partner
Model No. ¹	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Limited Set of Covariates plus Schooling²												
Schooling	-0.048*** (0.006)	-0.054*** (0.007)	-0.041*** (0.008)	-0.056*** (0.008)	-0.058*** (0.009)	-0.052*** (0.013)	-0.031*** (0.008)	-0.038*** (0.011)	-0.030*** (0.011)	-0.028** (0.011)	-0.029* (0.015)	-0.029* (0.016)
Observations	1604	981	623	1048	690	358	556	291	265	227	118	109
Log likelihood	-962.64	-575.59	-373.05	-646.24	-425.28	-215.57	-308.66	-149.82	-153.23	-128.20	-61.99	-65.28
Pseudo R-squared	0.05	0.08	0.05	0.06	0.07	0.06	0.04	0.07	0.04	0.06	0.09	0.05
Panel B: Limited Set of Covariates plus Educational Attainment Dummies²												
High School	0.039 (0.041)	0.099* (0.053)	-0.104 (0.071)	0.056 (0.062)	0.114 (0.076)	-0.097 (0.137)	0.029 (0.051)	0.083 (0.067)	-0.116 (0.088)	0.067 (0.081)	0.195** (0.085)	-0.026 (0.131)
Higher Education	-0.098** (0.045)	-0.107* (0.061)	-0.133* (0.076)	-0.103 (0.066)	-0.089 (0.083)	-0.180 (0.139)	-0.071 (0.061)	-0.107 (0.085)	-0.094 (0.101)	-0.083 (0.113)	-0.022 (0.127)	-0.133 (0.189)
College	-0.303*** (0.048)	-0.301*** (0.064)	-0.339*** (0.083)	-0.284*** (0.066)	-0.266*** (0.082)	-0.328** (0.140)	-0.317*** (0.089)	-0.393*** (0.128)	-0.443*** (0.123)	-0.412*** (0.121)	-0.282* (0.165)	-0.658*** (0.096)
Graduate	-0.296*** (0.056)	-0.290*** (0.073)	-0.354*** (0.099)	-0.289*** (0.074)	-0.263*** (0.093)	-0.411*** (0.138)	-0.245** (0.104)	-0.310** (0.131)	-0.135 (0.231)	-0.182 (0.142)	-0.150 (0.174)	0.158 (0.174)
Observations	1672	949	614	1101	666	355	571	280	257	227	112	105
Log likelihood	-981.10	-540.78	-367.22	-666.87	-401.24	-213.79	-308.26	-134.77	-146.58	-120.28	-53.10	-53.81
Pseudo R-squared	0.07	0.11	0.05	0.07	0.09	0.06	0.05	0.11	0.06	0.12	0.17	0.18

1. Probit estimations: coefficients are estimated marginal effects ($\partial F/\partial x_k$), i.e. the marginal effect on $\Pr(y=1)$, given a unit increase in the value of the relevant (continuous) regressor (x_k), holding all other regressors at their respective sample means. The discrete change in the probability is reported for binary regressors. Robust standard errors in parentheses. * $p<0.10$ ** $p<0.05$ *** $p<0.01$. Each model includes a full set of controls of the respective covariates set (coefficients not shown here). Cases weighted by the NES sample weight (v923008).

2. Limited Set of Covariates includes age, gender, and race.

Appendix: Summary Statistics and Variable Descriptions

Table A.1: Summary Statistics for NES Data

NES 1992					
Variable	Obs	Mean	Std. Dev.	Min	Max
Trade_Opinion	1736	0.674	0.464	0	1
Schooling	2329	12.978	2.733	0	17
Graduate	2420	0.076	0.264	0	1
College	2420	0.163	0.369	0	1
Higher Education	2420	0.242	0.428	0	1
High School	2420	0.343	0.475	0	1
Junior High	2329	0.036	0.187	0	1
Age	2485	44.271	17.210	17	91
Male	2485	0.477	0.500	0	1
White	2450	0.845	0.362	0	1
Black	2450	0.130	0.336	0	1
Indian	2450	0.012	0.110	0	1
Asian	2450	0.013	0.114	0	1
Trade Union Member	2472	0.108	0.310	0	1
Party ID	2445	2.727	2.022	0	6
Ideology	2277	0.791	0.938	0	2
NES 1996					
Trade_Opinion	846	0.524	0.491	0	1
Schooling	1713	13.432	2.581	0	17
Graduate	1711	0.099	0.299	0	1
College	1711	0.186	0.389	0	1
Higher Education	1711	0.274	0.446	0	1
High School	1711	0.315	0.465	0	1
Junior High	1713	0.021	0.143	0	1
Age	1712	45.529	16.853	18	93
Male	1714	0.479	0.500	0	1
White	1704	0.852	0.355	0	1
Black	1704	0.117	0.322	0	1
Indian	1704	0.012	0.108	0	1
Asian	1704	0.019	0.136	0	1
Trade Union Member	1687	0.120	0.325	0	1
Party ID	1695	2.735	2.101	0	6
Ideology	1651	0.759	0.922	0	2

Cases weighted by respective NES sample weight (v923008 or v960003).

Table A.2: Variable Descriptions for NES Data

Variable	Coding
Trade_Opinion	<i>Pro Free Trade Attitudes</i> : “Some people have suggested placing new limits on foreign imports in order to protect American jobs. Others say that such limits would raise consumer prices and hurt American exports. Do you favor or oppose placing new limits on imports, or haven’t you thought much about this?” Coded: 1=Favor New Limits, 0=Oppose. NES variables v923802 & v961327.
Schooling	Years of Full Time Education Completed. NES variables v923905 & v960607
Graduate	Highest Educational Attainment Dummy: Coded 1 if completed Graduate Education and 0 otherwise. NES variables v923908 & v960610.
College	Highest Educational Attainment Dummy: Coded 1 if completed College Education and 0 otherwise. NES variables v923908 & v960610.
Higher Education	Highest Educational Attainment Dummy: Coded 1 if completed some higher Education (Community or Junior College, etc.) and 0 otherwise. NES variables v923908 & v960610.
High School	Highest Educational Attainment Dummy: Coded 1 if completed High School Education and 0 otherwise. NES variables v923908 & v960610.
Junior High	Highest Educational Attainment Dummy: Coded 1 if completed only 8 years of schooling and 0 otherwise. NES variables v923905 & v960607.
Age	Age in years. NES variables v923903 & v960605.
Male	Gender: Coded 1 if Male and 0 if Female. NES variables v924201 & v960066.
White	Respondent’s Race: Coded 1 if White and 0 otherwise. NES variables v924202& v960067.
Black	Respondent’s Race: Coded 1 if Black and 0 otherwise. NES variables v924202& v960067.
Indian	Respondent’s Race: Coded 1 if Indian and 0 otherwise. NES variables v924202& v960067.
Asian	Respondent’s Race: Coded 1 if Asian and 0 otherwise. NES variables v924202& v960067.
Trade Union Member	Is respondent currently a Trade Union Member: Coded 1 if Yes and 0 if No. NES variables v924102/ v924101 & v960699/v960698.
Party ID	Respondent’s Party Identification: Coded from 0 “strong Democrat” to 6 “strong Republican.” NES variables v923634 & v960420
Ideology	Respondent’s Ideology: Coded 0 Conservative; 1 Moderate; 2 Liberal. NES variables v923513 & v960368.

Table A.3: Summary Statistics for ISSP Data

Variable	Obs	Mean	Std. Dev.	Min	Max
Pro-Trade-Dummy	30894	0.221	0.408	0	1
Against-Trade-Dummy	30894	0.570	0.495	0	1
Schooling	28022	11.375	3.713	0	20
Secondary Incomplete	30294	0.218	0.413	0	1
High School	30294	0.299	0.458	0	1
College	30294	0.230	0.421	0	1
Age	30666	44.355	16.923	14	98
Male	30778	0.480	0.500	0	1
Citizen	29440	0.973	0.163	0	1
Rural	24074	1.730	0.894	1	3
Party ID	18352	2.905	0.979	1	5
Trade Union Member	24662	0.302	0.459	0	1
LN Real Income	21860	9.215	1.700	2.5	13.9
LN GDP per capita	30894	9.487	0.668	8.1	10.2

Cases weighted by ISSP sample weight.

Table A.4: Variable Descriptions for ISSP Data

Variable Name	Coding:
Pro-Trade-Dummy	Pro Free Trade Attitudes: “How much do you agree or disagree with the following statements: (Respondent’s country) should limit the import of foreign products in order to protect its national economy.” Coded 1 if answered disagree strongly or disagree and 0 otherwise. ISSP variable (v38).
Against-Trade-Dummy	Pro Protectionism Attitudes: Same question as above but coded 1 if answered agree strongly or agree and 0 otherwise.
Schooling	Years of Full Time Education Schooling (cap at 20). Those with no formal schooling are coded as 0. ISSP variable (v204)
Secondary Incomplete	Highest educational attainment: Coded 1 if Incomplete Secondary Education and 0 otherwise. ISSP variable (v205)
High School	Highest educational attainment: Coded 1 if Completed Secondary Education and 0 otherwise. ISSP variable (v205)
College	Highest educational attainment: Coded 1 if had College/University Education or Semi-higher degrees and 0 otherwise. ISSP variable (v205)
Age	Age in years. ISSP variable (v201)
Male	Gender: Coded 1 if Male and 0 if Female. ISSP variable (v200)
Citizen	Citizenship: “Are you a citizen of (country)?” Coded 1 if Citizen and 0 otherwise. ISSP variable (v63)
Rural	Area of Residence: Coded 1 if Urban, 2 if Suburban, and 3 if Rural. ISSP variable (v295).
Party ID	Party Affiliation: Self Placement on a left – right scale. Coded from 1 “Far Left” to 5 “Far Right”. ISSP variable (v269).
Trade Union Member	Trade Union Membership: “Are you a member in a trade union at present?” Coded 1 if answered “Member Now” and 0 if answered “Not a Member Now”. ISSP variable (v268)
LN Real Income	Log of Real Income. Data in local currency converted by PPP conversion factors taken from WDI database. ISSP variable (v217).
LN GDP per capita	Log of GDP per capita 1995, PPP (current international dollars). Taken from WDI database.